victions ascribed by Pope to his untutored savage, that when he passed to the realms of the blessed "his faithful dog should bear him company." In fact, all these accessory questions to which I have referred involve problems which cannot be discussed by physical science, inasmuch as they do not lie within the scope of physical science, but come into the province of that great mother of all science, Philosophy. Before any direct answer can be given upon any of these questions we must hear what Philosophy has to say for or against the views that may be held. I need hardly say—especially having detained you so long as I find I have done—that I do not propose to enter into that region of discussion, and I might, properly enough, finish what I have to say upon the subject—especially as I have reached its natural limits—if it were not that an experience, now, I am sorry to say, extending over a good many years, leads me to anticipate that what I have brought before you to-night is not likely to escape the fate which, upon many occasions within my recollection, has attended statements of scientific doctrine and of the conclusions towards which science is tending, which have been made in a spirit intended at any rate to be as calm and as judicial as that in which I have now laid these facts before you. I do not doubt that the fate which has befallen better men will befall me, and that I shall have to bear in patience the reiterated assertion that doctrines such as I have put before you have very evil tendencies. I should not wonder if you were to be told by persons speaking with authority—not, perhaps, with that authority which is based upon knowledge and wisdom, but still with authority—that my intention in bringing this subject before you is to lead you to apply the doctrine I have stated, to man as well as brutes, and it will then certainly be further asserted that the logical tendency of such a doctrine is Fatalism, Materialism, and Atheism. Now, let me ask you to listen to another product of that long experience to which I referred. Logical consequences are very important; but in the course of my experience I have found that they are the scarecrows of fools and the beacons of wise men. Logical consequences can take care of themselves. The only question for any man to ask is—"Is this doctrine true, or is it false?" No other question can possibly be taken into consideration until that one is settled. And, as I have said, the logical consequences of doctrines can only serve as a warning to wise men to ponder well whether the doctrine submitted for their consideration be true or not, and to test it in every possible direction. Undoubtedly I do hold that the view I have taken of the relations between the physical and mental faculties of brutes applies in its fulness and entirety to man; and if it were true that the logical consequences of that belief must land me in all these terrible consequences, I should not hesitate in allowing myself to be so landed. I should conceive that if I refused I should have done the greatest and most abominable violence to everything which is deepest in my moral nature. But now I beg leave to say that, in my conviction, there is no such logical connection as is pretended between the doctrine I accept and the consequences which people profess to draw from it. Some years ago I had occasion, in dealing with the philosophy of Descartes, and some other matters, to state my conviction pretty fully on those subjects, and, although I know from experience how futile it is to endeavour to escape from those nicknames which many people mistake for argument, yet, if those who care to investigate these questions in a spirit of candour and justice will look into those writings of mine, they will see my reasons for not imagining that such conclusions can be drawn from such premises. To those who do not look into these matters with candour and with a desire to know the truth, I have nothing whatever to say, except to warn them on their own behalf what they do; for assuredly if, for preaching such doctrine as I have preached to you to-night, I am cited before the bar of public opinion, I shall not stand there alone. On my one hand I shall have, among theologians, St. Augustine, John Calvin, and a man whose name should be well known to the Presbyterians of Ulster—Jonathan Edwards—unless, indeed, it be the fashion to neglect the study of the great masters of divinity, as many other great studies are neglected nowadays; and I should have upon my other hand, among philosophers, Leibnitz; I should have Père Malebranche, who saw all things in God; I should have David Hartley, the theologian as well as philosopher; I should have Charles Bonnet, the eminent naturalist, and one of the most zealous defenders Christianity has ever had. I think I should have, within easy reach, at any rate, John Locke. Certainly the school of Descartes would be there, if not their master; and I am inclined to think that, in due justice, a citation would have to be served upon Immanuel Kant himself. In such society it may be better to be a prisoner than a judge; but I would ask those who are likely to be influenced by the din and clamour which are raised about these questions, whether they are more likely to be right in assuming that those great men I have mentioned—the fathers of the Church and the fathers of Philosophy—knew what they were about; or that the pigmies who raise the din know better than they did what they meant. It is not necessary for any man to occupy himself with problems of this kind unless he so choose. Life is full enough, filled to the brim, by the performance of its ordinary duties; but let me warn you, let me beg you to believe, that if a man elect to give a judgment upon these great questions; still more, if he assume to himself the responsibility of attaching praise or blame to his fellow-men for the judgments which they may venture to express—then, unless he would commit a sin more grievous than most of the breaches of the Decalogue, he must avoid a lazy reliance upon the information that is gathered by prejudice and filtered through passion. Let him go to those great sources that are open to him as to every one, and to no man more open than to an Englishman; let him go back to the facts of nature, and to the thoughts of those wise men who for generations past have been the interpreters of nature.

THE CARNIVOROUS HABITS OF PLANTS*

I HAVE chosen for the subject of my address to you from the chair in which the Council of the British Association has done me the honour of placing me, the carnivorous habits of some of our brother-organisms—Plants.

Various observers have described with more or less accuracy the habits of such vegetable sportsmen as the Sundew, the Venus's Fly-trap, and the Pitcher-plants, but few have inquired into their motives; and the views of those who have most accurately appreciated these have not met with that general acceptance which they deserved.

Quite recently the subject has acquired a new interest, from the researches of Mr. Darwin into the phenomena which accompany the placing albuminous substances on the leaves of Drosera and Pinguicula, and which, in the opinion of a very eminent physiologist, prove, in the case of Dionæa, that this plant digests exactly the same substances and in exactly the same way that the human stomach does. With these researches Mr. Darwin is still actively engaged, and it has been with the view of rendering him such aid as my position and opportunities at Kew afforded me, that I have, under his instructions, examined some other carnivorous plants.

In the course of my inquiries I have been led to look into the early history of the whole subject, which I find to be so little known and so interesting that I have thought that a sketch of it, up to the date of Mr. Darwin's investigations, might prove acceptable to the members of this Association. In drawing it up, I have been obliged to limit myself to the most important plants; and with regard to such of these as Mr. Darwin has studied, I leave it to him to announce the discoveries which, with his usual frankness, he has communicated to me and to other friends; whilst with regard to those which I have myself studied, Sarracenia and Nepenthes, I shall briefly detail such of my observations and experiments as seem to be the most suggestive.

Dionæa.—About 1768 Ellis, a well-known English naturalist, sent to Linnæus a drawing of a plant, to which he gave the poetical name of Dionæa. "In the year 1765," he writes, "our late worthy friend, Mr. Peter Collinson, sent me a dried specimen of this curious plant, which he had received from Mr. John Bartram, of Philadelphia, botanist to the late King." Ellis flowered the plant in his chambers, having obtained living specimens from America. I will read the account which he gave of it to Linnæus, and which moved the great naturalist to declare that, though he had seen and examined no small number of plants, he had never met with so wonderful a phenomenon:—

"The plant, Ellis says, shows that Nature may have some views towards its nourishment, in forming the upper joint of its leaf like a machine to catch food; upon the middle of this lies the bait for the unhappy insect that becomes its prey. Many minute red glands that cover its surface, and which perhaps discharge sweet liquor, tempts the animal to taste them; and the instant these tender parts are irritated by its feet, the two lobes rise up, grasp it fast, lock the rows of spines together, and squeeze it to death. And further, lest the strong efforts for life in the creature just taken should serve to disengage it, three

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small erect spines are fixed near the middle of each lobe, among the glands, that effectually put an end to all its struggles. Nor do the lobes ever open again, while the dead animal continues there. But it is nevertheless certain that the plant cannot distinguish an animal from a vegetable or mineral substance; for if we introduce a straw or pin between the lobes, it will grasp it fully as fast as if it was an insect."

This account, which in its way is scarcely less horrible than the descriptions of those mediæval statues which opened to embrace and stab their victims, is substantially correct, but erroneous in some particulars. I prefer to trace out our knowledge of the facts in historical order, because it is extremely important to realise in so doing how much our appreciation of tolerably simple matters may be influenced by the prepossessions that occupy our mind.

We have a striking illustration of this in the statement published by Linnæus a few years afterwards. All the facts which I have detailed to you were in his possession; yet he was evidently unable to bring himself to believe that Nature intended the plant—to use Ellis's words—" to receive some nourishment from the animals it seizes;" and he accordingly declared, that as soon as the insects ceased to struggle, the leaf opened and let them go. He only saw in these wonderful actions an extreme case of sensitiveness in the leaves, which caused them to fold up when irritated, just as the sensitive plant does; and he consequently regarded the capture of the disturbing insect as something merely accidental and of no importance to the plant. He was, however, too sagacious to accept Ellis's sensational account of the *coup de grace* which the insects received from the three stiff hairs in the centre of each lobe of the leaf.

Linnæus's authority overbore criticism, if any were offered; and his statements about the behaviour of the leaves were faithfully copied from book to book.

Broussonet (in 1784) attempted to explain the contraction of the leaves by supposing that the captured insect pricked them, and so let out the fluid which previously kept them turgid and expanded.

Dr. Darwin (1761) was contented to suppose that the Dionæa surrounded itself with insect traps to prevent depredations upon its flowers.

Sixty years after Linnæus wrote, however, an able botanist, the Rev. Dr. Curtis (dead but a few years since) resided at Wilmington, in North Carolina, the head-quarters of this very local plant. In 1834 he published an account of it in the *Boston Journal of Natural History*, which is a model of accurate scientific observation. This is what he said:—"Each half of the leaf is a little concave on the inner side, where are placed three delicate hair-like organs, in such an order that an insect can hardly traverse it without interfering with one of them, when the two sides suddenly collapse and enclose the prey, with a force surpassing an insect's efforts to escape. The fringe of hairs on the opposite sides of a leaf interlace, like the fingers of two hands clasped together. The sensitiveness resides only in these hair-like processes on the inside, as the leaf may be touched or pressed in any other part without sensible effects. The little prisoner is not crushed and suddenly destroyed, as is sometimes supposed, for I have often liberated captive flies and spiders, which sped away as fast as fear or joy could carry them. At other times I have found them enveloped in a fluid of a mucilaginous consistence, which seems to act as a solvent, the insects being more or less consumed in it."

To Ellis belongs the credit of divining the purpose of the capture of insects by the Dionæa. But Curtis made out the details of the mechanism, by ascertaining the seat of the sensitiveness in the leaves; and he also pointed out that the secretion was not a lure exuded before the capture, but a true digestive fluid poured out, like our own gastric juice after the ingestion of food.

For another generation the history of this wonderful plant stood still; but in 1868 an American botanist, Mr. Canby, who is happily still engaged in botanical research—while staying in the Dionæa district, studied the habits of the plant pretty carefully, especially the points which Dr. Curtis had made out. His first idea was that "the leaf had the power of dissolving animal matter, which was then allowed to flow along the somewhat trough-like petiole to the root, thus furnishing the plant with highly nitrogenous food." By feeding the leaves with small pieces of beef, he found, however, that these were completely dissolved and absorbed; the leaf opening again with a dry surface, and ready for another meal, though with an appetite somewhat jaded. He found that cheese disagreed horribly with the leaves, turning them black, and finally killing them. Finally, he details the useless struggles of a Curculio to escape, as thoroughly establishing the fact that the fluid already mentioned is actually secreted, and is not the result of the decomposition of the substance which the leaf has seized. The Curculio being of a resolute nature, attempted to eat his way out,—"when discovered he was still alive, and had made a small hole through the side of the leaf, but was evidently becoming very weak. On opening the leaf, the fluid was found in considerable quantity around him, and was without doubt gradually overcoming him. The leaf being again allowed to close upon him, he soon died."

At the meeting of this Association last year, Dr. Burdon-Sanderson made a communication, which, from its remarkable character, was well worthy of the singular history of this plant; one by no means closed yet, but in which his observations will head a most interesting chapter.

It is a generalisation—now almost a household word—that all living things have a common bond of union in a substance—always present where life manifests itself—which underlies all their details of structure. This is called *protoplasm*. One of its most distinctive properties is its aptitude to contract; and when in any given organism the particles of protoplasm are so arranged that they act as it were in concert, they produce a cumulative effect which is very manifest in its results. Such a manifestation is found in the contraction of muscle; and such a manifestation we possibly have also in the contraction of the leaf of Dionæa.

The contraction of muscle is well known to be accompanied by certain electrical phenomena. When we place a fragment of muscle in connection with a delicate galvanometer, we find that between the outside surface and a cut surface there is a definite current, due to what is called the electromotive force of the muscle. Now, when the muscle is made to contract, this electromotive force momentarily disappears. The needle of the galvanometer, deflected before, swings back towards the point of rest; there is what is called a *negative variation*. All students of the vegetable side of organised nature were astonished to hear from Dr. Sanderson that certain experiments which, at the instigation of Mr. Darwin, he had made, proved to demonstration that when a leaf of Dionæa contracts, the effects produced are precisely similar to those which occur when muscle contracts.

Not merely, then, are the phenomena of digestion in this wonderful plant like those of animals, but the phenomena of contractility agree with those of animals also.

Drosera.—Not confined to a single district in the New World, but distributed over the temperate parts of both hemispheres, in sandy and marshy places, are the curious plants called Sundews—the species of the genus Drosera. They are now known to be near congeners of Dionæa, a fact which was little more than guessed at when the curious habits which I am about to describe were first discovered.

Within a year of each other, two persons—one an Englishman, the other a German—observed that the curious hairs which everyone notices on the leaf of Drosera were sensitive.

This is the account which Mr. Gardom, a Derbyshire botanist, gives of what his friend Mr. Whateley, "an eminent London surgeon," made out in 1780:—"On inspecting some of the contracted leaves we observed a small insect or fly very closely imprisoned therein, which occasioned some astonishment as to how it happened to get into so confined a situation. Afterwards, on Mr. Whateley's centrically pressing with a pin other leaves yet in their natural and expanded form, we observed a remarkably sudden and elastic spring of the leaves, so as to become inverted upwards, and, as it were, encircling the pin, which evidently showed the method by which the fly came into its embarrassing situation."

This must have been an account given from memory, and represents the movement of the hairs as much more rapid than it really is.

In July of the preceding year (though the account was not published till two years afterwards), Roth, in Germany, had remarked in *Drosera rotundifolia* and *longifolia*, "that many leaves were folded together from the point towards the base, and that all the hairs were bent like a bow, but that there was no apparent change on the leaf-stalk." Upon opening these leaves, he says, "I found in each a dead insect; hence I imagined that this plant, which has some resemblance to the *Dionæa muscipula*, might also have a similar moving power."

"With a pair of pliers I placed an ant upon the middle of the leaf of *D. rotundifolia*, but not so as to disturb the plant The ant endeavoured to escape, but was held fast by the clammy juice at the points of the hairs, which was drawn out by its feet

into fine threads. In some minutes the short hairs on the disc of the leaf began to bend, then the long hairs, and laid themselves upon the insect. After a while the leaf began to bend, and in some hours the end of the leaf was so bent inwards as to touch the base. The ant died in fifteen minutes, which was before all the hairs had bent themselves."

These facts, established nearly a century ago by the testimony of independent observers, have up to the present time been almost ignored; and Trecul, writing in 1855, boldly asserted that the facts were not true.

More recently, however, they have been repeatedly verified: in Germany by Nitschke, in 1860; in America by a lady, Mrs. Treat, of New Jersey, in 1871; in this country by Mr. Darwin, and also by Mr. A. W. Bennett.

To Mr. Darwin, who for some years past has had the subject under investigation, we are indebted, not merely for the complete confirmation of the facts attested by the earliest observers, but also for some additions to those facts which are extremely important. The whole investigation still awaits publication at his hands, but some of the points which were established have been announced by Professor Asa Gray in America, to whom Mr. Darwin had communicated them.

Mr. Darwin found that the hairs on the leaf of Drosera responded to a piece of muscle or other animal substance, while to any particle of inorganic matter they were nearly indifferent. To minute fragments of carbonate of ammonia they were more responsive.

I will now give the results of Mrs. Treat's experiments, in her own words:—

"Fifteen minutes past ten I placed bits of raw beef on some of the most vigorous leaves of *Drosera longifolia*. Ten minutes past twelve two of the leaves had folded around the beef, hiding it from sight. Half-past eleven on the same day, I placed living flies on the leaves of *D. longifolia*. At twelve o'clock and forty-eight minutes, one of the leaves had folded entirely round its victim, and the other leaves had partially folded, and the flies had ceased to struggle. By half-past two, four leaves had each folded around a fly. The leaf folds from the apex to the petiole, after the manner of its vernation. I tried mineral substances, bits of dried chalk, magnesia, and pebbles. In twenty-four hours neither the leaves nor the bristles had made any move in clasping these articles. I wetted a piece of chalk in water, and in less than an hour the bristles were curving about it, but soon unfolded again, leaving the chalk free on the blade of the leaf."

Time will not allow me to enter into further details with respect to Dionæa and Drosera. The repeated testimony of various observers spreads over a century, and though at no time warmly received, must, I think, satisfy you that in this small family of the Droseraceæ we have plants which in the first place capture animals for purposes of food, and in the second, digest and dissolve them by means of a fluid which is poured out for the purpose; and thirdly, absorb the solution of animal matter which is so produced.

Before the investigations of Mr. Darwin had led other persons to work at the subject, the meaning of these phenomena was very little appreciated. Only a few years ago, Duchartre, a French physiological botanist, after mentioning the views of Ellis and Curtis with respect to Dionæa, expressed his opinion that the idea that its leaves absorbed dissolved animal substances was too evidently in disagreement with our knowledge of the function of leaves and the whole course of vegetable nutrition to deserve being seriously discussed.

Perhaps if the Droseraceæ were an isolated case of a group of plants exhibiting propensities of this kind, there might be some reason for such a criticism. But I think I shall be able to show you that this is by no means the case. We have now reason to believe that there are many instances of these carnivorous habits in different parts of the vegetable kingdom, and among plants which have nothing else in common but this.

As another illustration I shall take the very curious group of Pitcher-plants which is peculiar to the New World. And here also I think we shall find it most convenient to follow the historical order in the facts.

Sarracenia.—The genus Sarracenia consists of eight species, all similar in habit, and all natives of the Eastern States of North America, where they are found more especially in bogs, and even in places covered with shallow water. Their leaves, which give them a character entirely their own, are pitcher-shaped or trumpet-like, and are collected in tufts springing immediately from the ground; and they send up at the flowering season one or more slender stems bearing each a solitary flower. This has a singular aspect, due to a great extent to the umbrella like expansion in which the style terminates; the shape of this, or perhaps of the whole flower, caused the first English settlers to give to the plant the name of Side-saddle Flower.

Sarracenia purpurea is the best known species. About ten years ago it enjoyed an evanescent notoriety from the fact that its rootstock was proposed as a remedy for small-pox. It is found from Newfoundland southward to Florida, and is fairly hardy under open-air cultivation in the British Isles. At the commencement of the seventeenth century, Clusius published a figure of it, from a sketch which found its way to Lisbon and thence to Paris. Thirty years later Johnson copied this in his edition of Gerard's Herbal, hoping "that some or other that travel into foreign parts may find this elegant plant, and know it by this small expression, and bring it home with them, so that we may come to a perfecter knowledge thereof." A few years afterwards this wish was gratified. John Tradescant the younger found the plant in Virginia, and succeeded in bringing it home alive to England. It was also sent to Paris from Quebec by Dr. Sarrazin, whose memory has been commemorated in the name of the genus, by Tournefort.

The first fact which was observed about the pitchers was, that when they grew they contained water. But the next fact which was recorded about them was curiously mythical. Perhaps Morrison, who is responsible for it, had no favourable opportunites of studying them, for he declares them to be, what is by no means really the case, intolerant of cultivation (*respuere culturam videntur*).

He speaks of the lid, which in all the species is tolerably rigidly fixed, as being furnished, by a special act of providence, with a hinge. This idea was adopted by Linnæus, and somewhat amplified by succeeding writers, who declared that in dry weather the lid closed over the mouth, and checked the loss of water by evaporation. Catesby, in his fine work on the Natural History of Carolina, supposed that these water-receptacles might "serve as an asylum or secure retreat for numerous insects, from frogs and other animals which feed on them;"—and others followed Linnæus in regarding the pitchers as reservoirs for birds and other animals, more especially in times of drought; "*præbet aquam sitientibus aviculis.*"

The superficial teleology of the last century was easily satisfied without looking far for explanations, but it is just worth while pausing for a moment to observe that, although Linnæus had no materials for making any real investigation as to the purpose of the pitchers of Sarracenias, he very sagaciously anticipated the modern views as to their affinities. They are now regarded as very near allies of water-lilies—precisely the position which Linnæus assigned to them in his fragmentary attempt at a true natural classification. And besides this, he also suggested the analogy, which, improbable as it may seem at first sight, has been worked out in detail by Baillon (in apparent ignorance of Linnæus' writings) between the leaves of Sarracenia and water-lilies.

Linnæus seems to have supposed that Sarracenia was originally aquatic in its habits, that it had Nymphæa-like leaves, and that when it took to a terrestrial life its leaves became hollowed out, to contain the water in which they could no longer float—in fact, he showed himself to be an evolutionist of the true Darwinian type.

Catesby's suggestion was a very infelicitous one. The insects which visit these plants may find in them a retreat, but it is one from which they never return. Linnæus' correspondent Collinson remarked in one of his letters, that "many poor insects lose their lives by being drowned in these cisterns of water;" but William Bartram, the son of the botanist, seems to have been the first to put on record, at the end of the last century, the fact that Sarracenias catch insects and put them to death in the wholesale way that they do.

Before stopping to consider how this is actually achieved, I will carry the history a little further.

In the two species in which the mouth is unprotected by the lid it could not be doubted that a part, at any rate, of the contained fluid was supplied by rain. But in *Sarracenia variolaris*, in which the lid closes over the mouth, so that rain cannot readily enter it, there is no doubt that a fluid is secreted at the bottom of the pitchers, which probably has a digestive function. William Bartram, in the preface to his travels in 1791, described this fluid, but he was mistaken in supposing that it acted as a lure. There is a sugary secretion which attracts insects, but

this is only found at the upper part of the tube. Bartram must be credited with the suggestion, which he, however, only put forward doubtfully, that the insects were dissolved in the fluid, and then became available for the alimentation of the plants.

Sir J. E. Smith, who published a figure and description of *Sarracenia variolaris*, noticed that it secreted fluid, but was content to suppose that it was merely the gaseous products of the decomposition of insects that subserved the processes of vegetation. In 1829, however, thirty years after Bartram's book, Burnett wrote a paper containing a good many original ideas expressed in a somewhat quaint fashion, in which he very strongly insisted on the existence of a true digestive process in the case of Sarracenia, analogous to that which takes place in the stomach of an animal.

Our knowledge of the habits of *Sarracenia variolaris* is now pretty complete, owing to the observations of two South Carolina physicians. One, Dr. M'Bride, made his observations half a century ago, but they had, till quite recently, completely fallen into oblivion. He devoted himself to the task of ascertaining why it was that *Sarraeenia variolaris* was visited by flies, and how it was that it captured them. This is what he ascertained :—

"The cause which attracts flies is evidently a viscid substance resembling honey, secreted by or exuding from the internal surface of the tube. From the margin, where it commences, it does not extend lower than one-fourth of an inch. The falling of the insect as soon as it enters the tube is wholly attributable to the downward or inverted position of the hairs of the internal surface of the leaf. At the bottom of a tube split open, the hairs are plainly discernible, pointing downwards ; as the eye ranges upward they gradually become shorter and attenuated, till at or just below the surface covered by the bait they are no longer perceptible to the naked eye, nor to the most delicate touch. It is here that the fly cannot take a hold sufficiently strong to support itself, but falls."

Dr. Mellichamp, who is now resident in the district in which Dr. M'Bride made his observations, has added a good many particulars to our knowledge. He first investigated the fluid which is secreted at the bottom of the tubes. He satisfied himself that it was really secreted, and describes it as mucilaginous, but leaving in the mouth a peculiar astringency. He compared the action of this fluid with that of distilled water on pieces of fresh venison, and found that after fifteen hours the fluid had produced most change, and also most smell ; he therefore concluded that as the leaves when stuffed with insects become most disgusting in odour, we have to do, not with a true digestion, but with an accelerated decomposition. Although he did not attribute any true digestive power to the fluid secreted by the pitchers, he found that it had a remarkable anæsthetic effect upon flies immersed in it. He remarked that "a fly when thrown into water is very apt to escape, as the fluid seems to *run* from its wings," but it never escaped from the Sarracenia secretion. About half a minute after being thrown in, the fly became to all appearance dead, though, if removed, it gradually recovered in from half an hour to an hour.

According to Dr. Mellichamp, the sugary lure discovered by Dr. M'Bride, at the mouth of the pitchers, is not found on either the young ones of one season or the older ones of the previous year. He found, however, that about May it could be detected without difficulty, and more wonderful still, that there is a honey-baited pathway leading directly from the ground to the mouth, along the broad wing of the pitcher, up which insects are led to their destruction. From these narratives it is evident that there are two very different types of pitcher in Sarracenia, and an examination of the species shows that there may probably be three. These may be primarily classified into those with the mouth open and lid erect, and which consequently receive the rain-water in more or less abundance ; and those with the mouth closed by the lid, into which rain can hardly, if at all, find ingress.

To the first of these belongs the well-known *S. purpurea*, with inclined pitchers, and a lid so disposed as to direct all the rain that falls upon it also into the pitcher ; also *S. flava*, *rubra*, and *Drummonaii*, all with erect pitchers and vertical lids ; of these three, the lid in a young state arches over the mouth, and in an old state stands nearly erect, and has the sides so reflected that the rain which falls on its upper surface is guided down the outside of the back of the pitcher, as if to prevent the flooding of the latter.

To the second group belong *S. psittacina* and *S. variolaris*.

The tissues of the internal surfaces of the pitchers are singularly beautiful. They have been described in one species only, the *S. purpurea*, by August Vogl ; but from this all the other species which I have examined differ materially. Beginning from the upper part of the pitcher, there are four surfaces, characterised by different tissues, which I shall name and define as follows :—

1. An *attractive* surface, occupying the inner surface of the lid, which is covered with an epidermis, stomata, and (in common with the mouth of the pitcher) with minute honey-secreting glands ; it is further often more highly coloured than any other part of the pitcher, in order to attract insects to the honey.

2. A *conducting* surface, which is opaque, formed of glassy cells, which are produced into deflexed, short, conical, spinous processes. These processes, overlapping like the tiles of a house, form a surface down which an insect slips, and affords no foothold to an insect attempting to crawl up again.

3. A *glandular* surface (seen in *S. purpurea*), which occupies a considerable portion of the cavity of the pitcher below the conducting surface. It is formed of a layer of epidermis with sinuous cells, and is studded with glands ; and being smooth and polished, this too affords no foothold for escaping insects.

4. A *detentive* surface, which occupies the lower part of the pitcher, in some cases for nearly its whole length. It possesses no cuticle, and is studded with deflexed, rigid, glass-like, needle-formed, striated hairs, which further converge towards the axis of the diminishing cavity ; so that an insect, if once amongst them, is effectually detained, and its struggles have no other result than to wedge it lower and more firmly in the pitcher.

Now, it is a very curious thing that in *S. purpurea*, which has an open pitcher, so formed as to receive and retain a maximum of rain, no honey-secretion has hitherto been found, nor has any water been seen to be secreted in the pitcher ; it is, further, the only species in which (as stated above) I have found a special glandular surface, and in which no glands occur on the detentive surface. This concurrence of circumstances suggests the possibility of this plant either having no proper secretion of its own, or only giving it off after the pitcher has been filled with rain-water.

In *S. flava*, which has open-mouthed pitchers and no special glandular surface, I find glands in the upper portion of the detentive surface, among the hairs, but not in the middle or lower part of the same surface. It is proved that *S. flava* secretes fluid, but under what precise conditions I am not aware. I have found none but what may have been accidentally introduced in the few cultivated specimens which I have examined, either in the full-grown state, or in the half-grown when the lid arches over the pitcher. I find the honey in these as described by the American observers, and honey-secreting glands on the edge of the wing of the pitcher, together with similar glands on the outer surface of the pitcher, as seen by Vogl in *S. purpurea*.

Of the pitchers with closed mouths, I have examined those of *S. variolaris* only, whose tissues closely resemble those of *S. flava*. That it secretes a fluid noxious to insects there is no doubt, though in the specimens I examined I found none.

There is thus obviously much still to be learned with regard to Sarracenia, and I hope that American botanists will apply themselves to this task. It is not probable that three pitchers, so differently constructed as those of *S. flava*, *purpurea*, and *variolaris*, and presenting such differences in their tissues, should act similarly. The fact that insects normally decompose in the fluid of all, would suggest the probability that they all feed on the products of decomposition ; but as yet we are absolutely ignorant whether the glands within the pitchers are secretive, or absorptive, or both ; if secretive, whether they secrete water or a solvent ; and if absorptive, whether they absorb animal matter or the products of decomposition.

It is quite likely, that just as the saccharine exudation only makes its appearance during one particular period in the life of the pitcher, so the digestive functions may also be only of short duration. We should be prepared for this from the case of the Dionæa, the leaves of which cease after a time to be fit for absorption, and become less sensitive. It is quite certain that the insects which go on accumulating in the pitchers of Sarracenias must be far in excess of its needs for any legitimate process of digestion. They decompose ; and various insects, too wary to be entrapped themselves, seem habitually to drop their eggs into the open mouth of the pitchers, to take advantage of the accumulation of food. The old pitchers are consequently found to contain living larvæ and maggots, a sufficient proof that the original properties of the fluid which they secreted must have become exhausted ; and Barton tells us that various insectivorous

birds slit open the pitchers with their beaks to get at the contents. This was probably the origin of Linnæus' statement that the pitchers supplied birds with water.

The pitchers finally decay, and part, at any rate, of their contents must supply some nutriment to the plant by fertilising the ground in which it grows.

Darlingtonia.—I cannot take leave of Sarracenia without a short notice of its near ally, Darlingtonia, a still more wonderful plant, an outlier of Sarracenia in geographical distribution, being found at an elevation of 5,000 ft. on the Sierra Nevada of California, far west of any locality inhabited by Sarracenia. It has pitchers of two forms; one, peculiar to the infant state of the plant, consists of narrow, somewhat twisted, trumpet-shaped tubes, with very oblique open mouths, the dorsal lip of which is drawn out into a long, slender, arching, scarlet hood, that hardly closes the mouth. The slight twist in the tube causes these mouths to point in various directions, and they entrap very small insects only. Before arriving at a state of maturity the plant bears much larger, suberect pitchers, also twisted, with the lip produced into a large inflated hood, that completely arches over a very small entrance to the cavity of the pitcher. A singular orange-red, flabby, two-lobed organ hangs from the end of the hood, right in front of the entrance, which, as I was informed last week by letter from Prof. Asa Gray, is smeared with honey on its inner surface. These pitchers are crammed with large insects, especially moths, which decompose in them, and result in a putrid mass. I have no information of water being found in its pitchers in its native country, but have myself found a slight acid secretion in the young states of both forms of pitcher.

The tissues of the inner surfaces of the pitchers of both the young and the old plant I find to be very similar to those of *Sarracenia variolaris* and *flava*.

Looking at a flowering specimen of Darlingtonia, I was struck with a remarkable analogy between the arrangement and colouring of the parts of the leaf and of the flower. The petals are of the same colour as the flap of the pitcher, and between each pair of petals is a hole (formed by a notch in the opposed margins of each) leading to the stamens and stigma. Turning to the pitcher, the relation of its flap to its entrance is somewhat similar. Now, we know that coloured petals are specially attractive organs, and that the object of their colour is to bring insects to feed on the pollen or nectar, and in this case by means of the hole to fertilise the flower; and that the object of the flap and its sugar is also to attract insects, but with a very different result, cannot be doubted. It is hence conceivable that this marvellous plant lures insects to its flowers for one object, and feeds them while it uses them to fertilise itself, and that, this accomplished, some of its benefactors are thereafter lured to its pitchers for the sake of feeding itself!

But to return from mere conjecture to scientific earnest, I cannot dismiss Darlingtonia without pointing out to you what appears to me a most curious point in its history; which is, that the change from the slender, tubular, open-mouthed to the inflated closed-mouthed pitchers is, in all the specimens which I have examined, absolutely sudden in the individual plant. I find no pitchers in an intermediate stage of development. This, a matter of no little significance in itself, derives additional interest from the fact that the young pitchers to a certain degree represent those of the Sarracenias with open mouths and erect lids; and the old pitchers those of the Sarracenias with closed mouths and globose lids. The combination of representative characters in an outlying species of a small order cannot but be regarded as a marvellously significant fact in the view of those morphologists who hold the doctrine of evolution.

Nepenthes.—The genus Nepenthes consists of upwards of thirty species of climbing, half shrubby plants, natives of the hotter parts of the Asiatic Archipelago from Borneo to Ceylon, with a few outlying species in New Caledonia, in Tropical Australia, and in the Seychelle Islands on the African coast. Its pitchers are abundantly produced, especially during the younger state of the plants. They present very considerable modifications of form and external structure, and vary greatly in size, from little more than an inch to almost a foot in length; one species, indeed, which I have here from the mountains of Borneo, has pitchers which, including the lid, measure a foot and a half, and its capacious bowl is large enough to drown a small animal or bird.

The structure of the pitcher of Nepenthes is less complicated on the whole than that of Sarracenia, though some of its tissues are much more highly specialised. The pitcher itself is here not a transformed leaf, as in Sarracenia, nor is it a transformed leaf-blade, like that of Dionæa, but an appendage of the leaf developed at its tip, and answers to a water-secreting gland that may be seen terminating the mid-rib of the leaf of certain plants. It is furnished with a stalk, often a very long one, which in the case of pitchers formed on leaves high up the stem has (before the full development of the pitcher) the power of twisting like a tendril round neighbouring objects, and thus aiding the plant in climbing, often to a great height in the forest.

In most species the pitchers are of two forms, one appertaining to the young, the other to the old state of the plant, the transition from one form to the other being gradual. Those of the young state are shorter and more inflated; they have broad fringed longitudinal wings on the outside, which are probably guides to lead insects to the mouth; the lid is smaller and more open, and the whole interior surface is covered with secreting glands. Being formed near the root of the plant, these pitchers often rest on the ground, and in species which do not form leaves near the root they are sometimes suspended from stalks which may be fully a yard long, and which bring them to the ground. In the older state of the plant the pitchers are usually much longer, narrower, and less inflated, and are trumpet-shaped, or even conical; the wings also are narrower, less fringed, or almost absent. The lid is larger and slants over the mouth, and only the lower part of the pitcher is covered with secreting glands, the upper part presenting a tissue analogous to the conducting tissue of Sarracenia, but very different anatomically. The difference in structure of these two forms of pitcher, if considered in reference to their different positions on the plant, forces the conclusion on the mind that the one form is intended for ground game, the other for winged game. In all cases the mouth of the pitcher is furnished with a thickened corrugated rim, which serves three purposes: it strengthens the mouth and keeps it distended; it secretes honey (at least in all the species I have examined under cultivation, for I do not find that any other observer has noticed the secretion of honey by Nepenthes), and it is in various species developed into a funnel-shaped tube that descends into the pitcher and prevents the escape of insects, or into a row of incurved hooks that are in some cases strong enough to retain a small bird, should it, when in search of water or insects, thrust its body beyond a certain length into the pitcher.

In the interior of the pitcher of Nepenthes there are three principal surfaces: an *attractive*, *conductive*, and a *secretive* surface; the *detentive* surface of Sarracenia being represented by the fluid secretion, which is here invariably present at all stages of growth of the pitcher.

The attractive surfaces of Nepenthes are two: those, namely, of the rim of the pitcher, and of the under surface of the lid, which is provided in almost every species with honey-secreting glands, often in great abundance. These glands consist of spherical masses of cells, each embedded in a cavity of the tissue of the lid, and encircled by a guard-ring of glass-like cellular tissue. As in Sarracenia, the lid and mouth of the pitcher are more highly coloured than any other part, with the view of attracting insects to their honey. It is a singular fact that the only species known to me that wants these honey-glands on the lid is the *N. ampullaria*, whose lid, unlike that of the other species, is thrown back horizontally. The secretion of honey on a lid so placed would tend to lure insects away from the pitcher instead of into it.

From the mouth to a variable distance down the pitcher is an opaque glaucous surface, precisely resembling in colour and appearance the conductive surface of the Sarracenia, and, like it, affording no foothold to insects, but otherwise wholly different; it is formed of a fine network of cells, covered with a glass-like cuticle, and studded with minute reniform transverse excrescences.

The rest of the pitcher is entirely occupied with the secretive surface, which consist of a cellular floor crowded with spherical glands in inconceivable numbers. Each gland precisely resembles a honey-gland of the lid, and is contained in a pocket of the same nature, but semicircular, with the mouth downwards, so that the secretive fluid all falls to the bottom of the pitcher. In the *Nepenthes Rafflesiana* 3,000 of the glands occur on a square inch of the inner surface of the pitcher, and upwards of 1,000,000 in an ordinary sized pitcher. I have ascertained that, as was indeed to be expected, they secrete the fluid which is contained in the bottom of the pitcher before this opens, and that the fluid is always acid.

The fluid, though invariably present, occupies a comparatively small portion of the glandular surface of the pitcher, and is collected before the lid opens. When the fluid is emptied out of a

fully formed pitcher that has not received animal matter, it forms again, but in comparatively very small quantities; and the formation goes on for many days, and to some extent even after the pitcher has been removed from the plant. I do not find that placing inorganic substances in the fluid causes an increased secretion, but I have twice observed a considerable increase of fluid in pitchers after putting animal matter in the fluid.

To test the digestive powers of Nepenthes I have closely followed Mr. Darwin's treatment of Dionæa and Drosera, employing white of egg, raw meat, fibrine, and cartilage. In all cases the action is most evident, in some surprising. After twenty-four hours' immersion the edges of the cubes of white of egg are eaten away and the surfaces gelatinised. Fragments of meat are rapidly reduced; and pieces of fibrine weighing several grains dissolve and totally disappear in two or three days. With cartilage the action is most remarkable of all; lumps of this weighing 8 or 10 grains are half gelatinised in twenty-four hours, and in three days the whole mass is greatly diminished, and reduced to a clear transparent jelly. After drying some cartilage in the open air for a week, and placing it in an unopened but fully formed pitcher of *N. Rafflesiana*, it was acted upon similarly and very little slower.

That this process, which is comparable to digestion, is not wholly due to the fluid first secreted by the glands, appears to me most probable; for I find that very little action takes place in any of the substances placed in the fluid drawn from pitchers, and put in glass tubes; nor has any followed after six days' immersion of cartilage or fibrine in pitchers of *N. ampullaria* placed in a cold room; whilst on transferring the cartilage from the pitcher of *N. ampullaria* in the cold room to one of *Rafflesiana* in the stove, it was immediately acted upon. Comparing the action of fibrine, meat, and cartilage placed in tubes of Nepenthes fluid, with others in tubes of distilled water, I observed that their disintegration is three times more rapid in the fluid; but this disintegration is wholly different from that effected by immersion in the fluid of the pitcher of a living plant.

In the case of small portions of meat, $\frac{1}{2}$ to 2 grains, all seem to be absorbed; but with 8 to 10 grains of cartilage it is not so—a certain portion disappears, the rest remains as a transparent jelly, and finally becomes putrid, but not till after many days. Insects appear to be acted upon somewhat differently, for after several days' immersion of a large piece of cartilage I found that a good-sized cockroach, which had followed the cartilage and was drowned for his temerity, in two days became putrid. In removing the cockroach the cartilage remained inodorous for many days. In this case no doubt the antiseptic fluid had permeated the tissue of the cartilage, whilst enough did not remain to penetrate the chitinous hard covering of the insect, which consequently decomposed.

In the case of cartilage placed in fluid taken from the pitcher—it becomes putrid, but not so soon as if placed in distilled water.

From the above observations it would appear probable that a substance acting as pepsine is given off from the inner wall of the pitcher, but chiefly after placing animal matter in the acid fluid; but whether this active agent flows from the glands or from the cellular tissue in which they are imbedded, I have no evidence to show.

I have here not alluded to the action of these animal matters in the cells of the glands, which is, as has been observed by Mr. Darwin in Drosera, to bring about remarkable changes in their protoplasm, ending in their discoloration. Not only is there aggregation of the protoplasm in the gland-cells, but the walls of the cells themselves become discoloured, and the glandular surface of the pitcher that at first was of a uniform green, becomes covered with innumerable brown specks (which are the discoloured glands). After the function of the glands is exhausted, the fluid evaporates, and the pitcher slowly withers.

At this stage I am obliged to leave this interesting investigation. That Nepenthes possesses a true digestive process such as has been proved in the case of Drosera, Dionæa, and Pinguicula, cannot be doubted. This process, however, takes place in a fluid which deprives us of the power of following it further by direct observation. We cannot here witness the pouring out of the digestive fluid; we must assume its presence and nature from the behaviour of the animal matter placed in the fluid in the pitcher. From certain characters of the cellular tissues of the interior walls of the pitcher, I am disposed to think that it takes little part in the processes of either digestion or assimilation, and that these, as well as the pouring out of the acid fluid, are all functions of the glands.

In what I have said I have described the most striking instances of plants which seem to invert the order of nature, and to draw their nutriment—in part, at least—from the animal kingdom, which it is often held to be the function of the vegetable kingdom to sustain.

I might have added some additional cases to those I have already dwelt upon. Probably, too, there are others still unknown to science, or whose habits have not yet been detected. Delpino, for example, has suggested that a plant, first described by myself in the Botany of the Antarctic Voyage, *Caltha dionæafolia*, is so analogous in the structure of its leaves to Dionæa, that it is difficult to resist the conviction that its structure also is adapted for the capture of small insects.

But the problem that forces itself upon our attention is, How does it come to pass that these singular aberrations from the otherwise uniform order of vegetable nutrition make their appearance in remote parts of the vegetable kingdom? why are they not more frequent, and how were such extraordinary habits brought about or contracted? At first sight the perplexity is not diminished by considering—as we may do for a moment—the nature of ordinary vegetable nutrition. Vegetation, as we see it everywhere, is distinguished by its green colour, which we know depends on a peculiar substance called chlorophyll, a substance which has the singular property of attracting to itself the carbonic acid gas which is present in minute quantities in the atmosphere, of partly decomposing it, so far as to set free a portion of its oxygen, and of recombining it with the elements of water, to form those substances, such as starch, cellulose, and sugar, out of which the framework of the plant is constructed.

But, besides these processes, the roots take up certain matters from the soil. Nitrogen forms nearly four-fifths of the air we breathe, yet plants can possess themselves of none of it in the free uncombined state. They withdraw nitrates and salts of ammonia in minute quantities from the ground, and from these they build up with starch, or some analogous material, albuminoids or protein compounds, necessary for the sustentation and growth of protoplasm.

At first sight nothing can be more unlike this than a Dionæa or a Nepenthes capturing insects, pouring out a digestive fluid upon them, and absorbing the albuminoids of the animal, in a form probably directly capable of appropriation for their own nutrition. Yet there is something not altogether wanting in analogy in the case of the most regularly constituted plants. The seed of the castor-oil plant contains, besides the embryo seedling, a mass of cellular tissue or endosperm filled with highly nutritive substances. The seedling lies between masses of this, and is in contact with it; and as the warmth and moisture of germination set up changes which bring about the liquefaction of the contents of the endosperm and the embryo absorbs them, it grows in so doing, and at last, having taken up all it can from the exhausted endosperm, develops chlorophyll in its cotyledons under the influence of light, and relies on its own resources.

A large number of plants, then, in their young condition, borrow their nutritive compounds ready prepared; and this is in effect what carnivorous plants do later in life.

That this is not a merely fanciful way of regarding the relation of the embryo to the endosperm, is proved by the ingenious experiments of Van Tieghem, who has succeeded in substituting for the real, an artificial endosperm, consisting of appropriate nutritive matters. Except that the embryo has its food given to it in a manner which needs no digestion—a proper concession to its infantine state—the analogy here with the mature plants which feed on organic food seems to be complete.

But we are beginning also to recognise the fact that there are a large number of flowering plants that pass through their lives without ever doing a stroke of the work that green plants do. These have been called Saprophytes. Monotropra, the curious bird's nest orchis (*Neottia nidus-avis*), Epipogium, and Corallorhiza are instances of British plants which nourish themselves by absorbing the partially decomposed materials of other plants, in the shady or marshy places which they inhabit. They reconstitute these products of organic decomposition, and build them up once more into an organism. It is curious to notice, however, that the tissues of Neottia still contain chlorophyll in a nascent though useless state, and that if a plant of it be immersed in boiling water, the characteristic green colour reveals itself.

Epipogium and Corallorhiza have lost their proper absorbent organs; they are destitute of roots, and take in their food by the surfaces of their underground stem structures.

The absolute difference between plants which absorb and nourish themselves by the products of the decomposition of plant-structures, and those which make a similar use of animal structures, is not very great. We may imagine that plants accidentally permitted the accumulation of insects in some parts of their structure, and the practice became developed because it was found to be useful. It was long ago suggested that the receptacle formed by the connate leaves of Dipsacus might be an incipient organ of this kind; and though no insectivorous habit has ever been brought home to that plant, the theory is not improbable.

Linnæus, and more lately Baillon, have shown how a pitcher of Sarracenia may be regarded as a modification of a leaf of the Nymphæa type. We may imagine such a leaf first becoming hollow, and allowing *débris* of different kinds to accumulate; these would decompose, and a solution would be produced, some of the constituents of which would diffuse themselves into the subjacent plant tissues. This is in point of fact absorption, and we may suppose that in the first instance—as perhaps still in *Sarracenia purpurea*—the matter absorbed was merely the saline nutritive products of decomposition, such as ammoniacal salts. The act of digestion—that process by which soluble food is reduced without decomposition to a soluble form fitted for absorption—was doubtless subsequently acquired.

The secretion, however, of fluids by plants is not an unusual phenomenon. In many Aroids a small gland at the apex of the leaves secretes fluid, often in considerable quantities, and the pitcher of Nepenthes is, as I have shown elsewhere, only a gland of this kind, enormously developed. May not, therefore, the wonderful pitchers and carnivorous habit of Nepenthes have both originated by natural selection out of one such honey-secreting gland as we still find developed near that part of the pitcher which represents the tip of the leaf? We may suppose insects to have been entangled in the viscid secretion of such a gland, and to have perished there, being acted upon by those acid secretions that abound in these and most other plants. The subsequent differentiation of the secreting organs of the pitcher into aqueous, saccharine, and acid, would follow *pari passu* with the evolution of the pitcher itself, according to those mysterious laws which result in the correlation of organs and functions throughout the kingdom of Nature; and which, in my apprehension, transcend in wonder and interest those of evolution and the origin of species.

Delpino has recorded the fact that the spathe of Alocasia secretes an acid fluid which destroys the slugs that visit it, and which he believes subserves its fertilisation. Here any process of nutrition can only be purely secondary. But the fluids of plants are in the great majority of cases acid, and, when exuded, would be almost certain to bring about some solution in substances with which they came in contact. Thus the acid secretions of roots were found by Sachs to corrode polished marble surfaces with which they came in contact, and thus to favour the absorption of mineral matter.

The solution of albuminoid substances requires, however, besides a suitable acid, the presence of some other albuminoid substance analogous to pepsine. Such substances, however, are frequent in plants. Besides the well-known diastase, which converts the starch of malt into sugar, there are other instances in the synaptase which determines the formation of hydrocyanic acid from emulsine, and the myrosin which similarly induces the formation of oil of mustard. We need not wonder, then, if the fluid secreted by a plant should prove to possess the ingredien s necessary for the digestion of insoluble animal matters.

These remarks will, I hope, lead you to see, that though the processes of plant nutrition are in general extremely different from those of animal nutrition, and involve very simple compounds, yet that the protoplasm of plants is not absolutely prohibited from availing itself of food, such as that by which the protoplasm of animals is nourished; under which point of view these phenomena of carnivorous plants will find their place, as one more link in the continuity of nature.

BRITISH ASSOCIATION REPORTS

Report of the Committee on Mathematical Tables.

The objects for which the Committee were appointed at Edinburgh were twofold, viz., the preparation of a list of tables scattered about in books and mathematical journals and transactions, and the calculation of new tables. With regard to the first object, the tables were roughly divided into three classes, viz. (1) ordinary tables (such as trigonometrical and logarithmic) usually published in books; (2) tables of continuously varying quantities, generally definite integrals; and (3) theory-of-numbers tables. On the first class Mr. J. W. L. Glaisher had already written a report, to which it was intended, after the lapse of several years, to add a supplement; with the second some progress had been made; while Prof. Cayley proposed to undertake the third. The Committee had to acknowledge the assistance of several foreigners, and chiefly of Prof. Bierens de Haan, who had forwarded to them an account of 128 logarithmic and 105 non-logarithmic tables; to Dr. Carl Ohrtmann, of Berlin; and Profs. W. W. Johnson and J. M. Rice, of Annapolis, Maryland. The principal achievement, however, which the Committee had to report related to the second object, and was the completion of the tables of the Elliptic Functions, the commencement of which was noticed in NATURE nearly two years ago, and on which six or seven computers, under the superintendence of Mr. J. Glaisher, F.R.S., and Mr. J. W. L. Glaisher, have since been constantly engaged. These tables (which are of double entry) give the four theta functions, which form the numerators and denominators of the three elliptic functions, and their logarithms for 8,100 arguments; so that they contain nearly 65,000 tabular results. The calculation has been carried to ten figures, but only eight will be printed, the tabular portion of the work occupying 360 pages. Parts of the introduction will be written by Prof. Cayley, Sir William Thomson, and Prof. H. J. S. Smith, and it is hoped that before the next meeting of the Association the whole work, which will form one of the largest tables that have appeared as the result of an original calculation, will be in print. It is perhaps desirable to state that the elliptic functions which have thus been tabulated are, as it were, generalised sines and cosines. Sines and cosines may be combined so as to represent any singly periodic function, as is well known; and in the same way elliptic functions represent every possible doubly periodic function; and no quantities can be of a higher degree of periodicity. The elliptic functions (which are in a sense inverse to Legendre's Elliptic Integrals) are thus quantities of the highest importance and generality in mathematics, and they are daily becoming of more importance in physics. They appear conspicuously in the investigation of the motion of a rigid body and in electrostatics, and have also numerous applications in the theory of numbers. The calculations were just completed before the meeting, and the printing will commence immediately: it is intended that the tables shall be stereotyped to ensure freedom from typographical errors.

Report of the Committee on the Nomenclature of Dynamical and Electrical Units.

They have circulated numerous copies of their last year's report among scientific men both at home and abroad. They believe, however, that in order to render their recommendations fully available for science teaching and scientific work, a full and popular exposition of the whole subject of physical units is necessary, together with a collection of examples (tabular and otherwise) illustrating the application of systematic units to a variety of physical measurements. Students usually find peculiar difficulty in questions relating to units; and even the experienced scientific calculator is glad to have before him concrete examples with which to compare his own results as a security against misapprehension or mistake.

Some members of the Committee have been preparing a small volume of Illustrations of the C. G. S. System (centimetre-gramme-second system) intended to meet this want. The Committee do not desire to be re-appointed; at all events at present.

On Siemens' Pyrometer, by Prof. G. C. Foster.

The committee appointed to report upon Siemens' pyrometer has sought to determine whether or no the resistance is altered after exposure to high temperatures. The resistance was measured by means of Wheatstone's Bridge. An arrangement was adopted whereby the heat of the connecting wires was prevented from affecting the measurements. As a long thick iron tube surrounded the platinum coil of the pyrometer, it was impossible, in order to secure a standard temperature, to plunge the instrument into ice-cold water, because, owing to the conductivity of the iron, there was no certainty that the pyrometer wire was actually at the same temperature as the water. The temperature of 10°, which was near the usual atmospheric temperature, was adopted as the standard.

Four instruments were examined: in one of them (1) the coil was surrounded by an iron sheath, in (2) and (3) a piece of stout platinum foil surrounded the cylinder between the iron sheath and the coil. In (4) there was no iron sheath, but a platinum